LONG ISLAND BIOLOGICAL ASSOCIATION
COLD SPRING HARBOR, NEW YORK

THE BIOLOGICAL LABORATORY

November 29, 1956

Dear Josh,

I have made a few comments and rewritten one section (insert I). However, I don't feel very strongly about these points and will be willing to let you decide which changes are worth making. I do feel that a little more detail on the method would be helpful -perhaps not in the form I suggest. I am a little confused myself. Almost invariably, when working with NGA plates, I transferred daily so that I have little idea what would happen if the first plate had been allowed to incubate indefinitely. My feeling is that it would have dried up badly before any good swarms had a chance to form. Weren't most of your motilizations in tubes? Or did you prevent evaporation better than I?

Comment A. p.4. Altho both components in each mixture were quite motile and the objection is minimized, it's still true (as you imply) that only a few of the inoculated cells may contribute to the swarm periphery. -So that the results could be fortuitous (from 1 expt). Couldn't something of the following nature be said: "Qualitatively similar results were obtained in ___ repetitions of this experiment; in each instance, the final Gal-/Gal+ ratio was markedly higher in the Gal+ F+ x Gal- F- mixture than in the mixture involving the reverse coupling."

(over)

The experiments are very nice; altho, ofcourse, a little disappointing.

If you or Alan make no additional major changes (of your MS) I don't think it will be necessary for you to send it to me again, if this would be especially inconvenient.

Rumor has it that you are going to Australia?

Best regards to everyone.

Dave